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Program Options to Explore Ocean Worlds

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Abstract

Including Earth, roughly a dozen water ocean worlds exist in the solar system: the relict worlds Ceres and Mars, vast oceans inside most of the large Jovian and Saturnian icy moons, and Kuiper Belt Objects like Triton, Charon, and Pluto whose geologies are dominated by water and ammonia. Key pieces of the ocean-world science puzzle – which when completed may reveal whether life is widespread in the cosmos, why it exists where it does, and how it originates – are distributed among them. The eventual exploration of all these worlds will yield humanity's total tangible knowledge about life in the universe, essentially forever. Thus, their exploration has existential significance for humanity's self-regard, and indeed perhaps of our place in the natural scheme. The matter of planning how to pursue such a difficult and unprecedented exploration opportunity is therefore historic. The technical challenges are formidable, far harder than at Mars: missions to the Jovian and Saturnian ocean worlds are severely power-limited; trip times can be as much as a half decade and decade, respectively. And the science targets are global-scale oceans beneath kilometers of cryogenic ice. Reaching and exploring them would be a multi-generational undertaking, so again it is essential to plan and prepare. Today, we lack the instrumentation, subsystems, and remote operational-intelligence technologies needed to build and use exploration avatars as good as what we can envision needing.

Each ocean world holds a piece of the puzzle, but the three priority targets are Europa at Jupiter, and Enceladus and Titan at Saturn. As with the systematic exploration of Mars, exploring these diverse worlds poses a complex technical and programmatic challenge – a strategic challenge – that needs to be designed and managed if each generation is to see its work bear fruit, and if the space science community is to make most effective use of the public money devoted to the quest. Strategic programs benefit from coherence. In only 15 years, the Mars Exploration Program (MEP) has transformed humanity's view of Mars as a once and future habitable place, a world quite possibly holding relict evidence of life. Finding such evidence, we would study it to know if that life shared an origin common with Earth life. However, life in the ocean worlds could not have shared our origin, so exploring them opens another level in our quest to understand life in the universe: not only to places with vast salt-water seas known to contain organics and hydrothermal seafloors active today, but to places where anything alive cannot be related to us.

MEP's success – from its presence in the public consciousness to its rewriting of planetary habitability – make it an obvious template and source of lessons learned for a viable ocean worlds exploration program (OWEP). Six attributes of the MEP are analyzed for application to a potential OWEP. From this, five hypothetical programmatic scenarios are compared to the default case, and conclusions drawn. A coherent OWEP should have several parts: first, dedicated continuous investment in enabling technologies; and second, two directed-purpose, medium-class (~$1B) missions per decade that conduct pivotal investigations on a documented roadmap. Science could start in 2035, informing development of decadal flagship missions after Europa Clipper, to the places revealed to hold the most promise. The fastest pace of scientific discoveries would require access to high-performance propulsion infrastructure, e.g., the Space Launch System, Falcon Heavy, and high-power in-space solar electric propulsion, all capable of halving trip time. Not including these boosts, such a program would cost about a half-billion dollars more per year than NASA's existing mission portfolio; the program architecture funded today cannot deliver a strategic OWEP while also sustaining balance among other solar system exploration priorities and opportunities.

Follow the Water. Yes, into the Ocean Worlds.

Keywords: ocean worlds, astrobiology, icy moons, Europa, Enceladus, Titan

Introduction

Today we know of at least ten Ocean Worlds in our solar system, including Earth of course but otherwise a quite diverse set. To learn the limits of life in these places – the potentially habitable worlds nearby – we have to explore them all.

This will take many decades to do. Having a clear strategy is essential if we are to prioritize among the feasible options, and make the smartest investments.

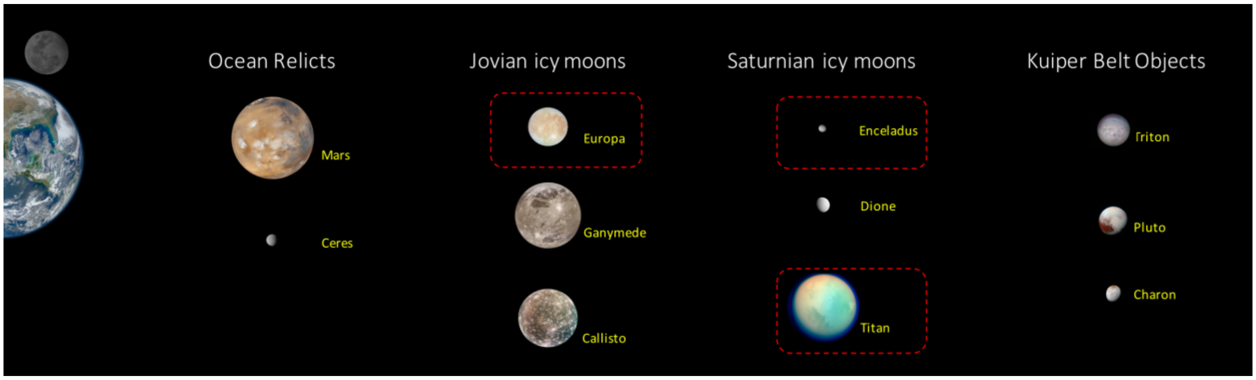

Figure 1. About a dozen ocean worlds exist in the solar system, with diverse environmental conditions and states. They are the only planetary destinations available for humanity to learn the limits of life in tangible detail. Three have special priority, for diverse reasons.

Reference [1] discusses how to scientifically identify, confirm, and study worlds that may have, or may have had, oceans. Reference [2] surveys the phenomenology of ocean worlds throughout the solar system. Figure 1 shows an ocean-world taxonomy and highlights the 'ocean-world starter set' among them: the three most likely to reveal the most, soonest, about the extent and diversity of life in our solar system.

Europa

Europa, second-highest flagship mission priority of this decade [3] and the target of NASA's planned Europa Clipper, ought to be habitable (Figure 2). Almost as big as Earth's Moon, it has an internal ocean with twice as much water as all of Earth's seas, and likely a hydrothermal seafloor (Figure 3). The ice crust enclosing the ocean, of indeterminate thickness, is nonetheless geologically young, with ample evidence of dramatic tectonics that imply opportunities for exchange between ocean and surface over geologic time (Figure 4). The surface is irradiated by Jupiter, likely generating oxidants that get cycled into the ocean over geologic timescales.

Europa is our solar system's intrinsically most promising home for an alien ecology unrelated to Earth life. The next step of exploration would be a comprehensive investigation of the moon's habitability,

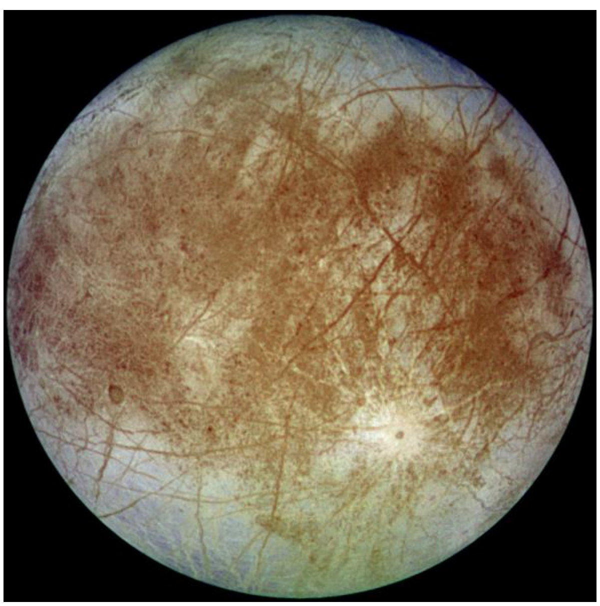

Figure 2. Europa has one of the youngest surfaces in the solar system, an ice shell enclosing a salt-water ocean about twice as large as Earth's.

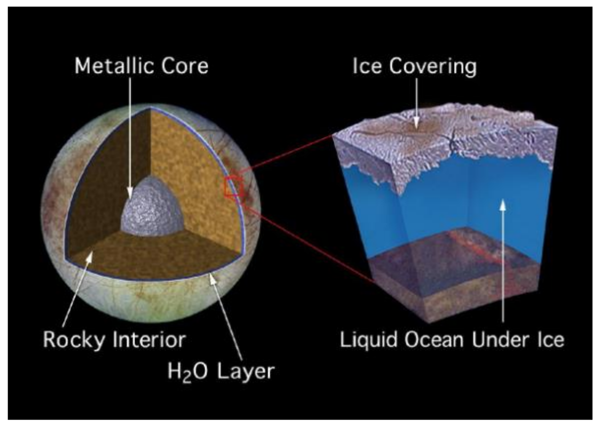

Figure 3. The floor of Europa's ocean is silicate rock, and hydrothermal activity is likely.

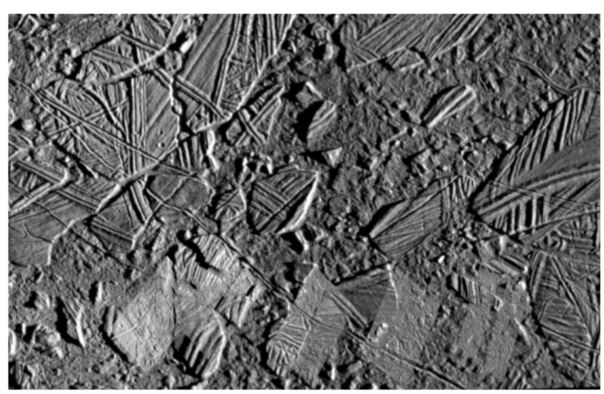

Figure 4. Significant exchange between Europan ocean and surface is strongly implied by chaos morphology. Image: NASA/JPL/Univ AZ.

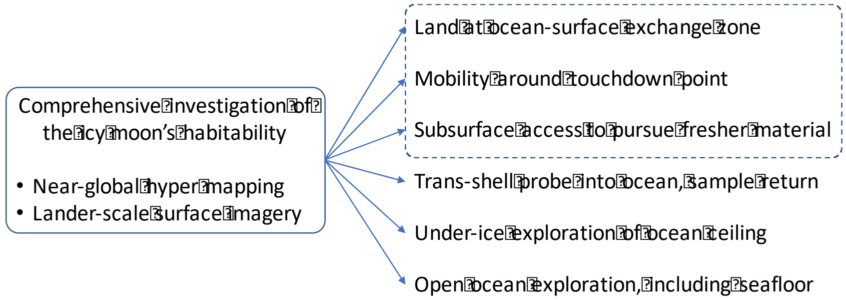

Figure 5. Europa program in three steps. Europa ought to be habitable; but its large size and Jupiter's strong radiation make it a tough place to explore. The Europa Clipper flagship would establish the basis (solid box). First generation Europa landers with today's technology could determine the history of the ocean's chemistry, including possible biosignatures (dashed box). Open ocean exploration would require development of several new, mission-scale technologies.

which is indeed the purpose of Europa Clipper. Subsequent steps could include: 1) landing at a confirmed ocean-surface exchange zone, to access ocean material deposited near and on the surface; 2) local mobility in such a zone, to explore diverse material types and emplacements; 3) subsurface access at such a site, in pursuit of increasingly fresh material; 4) examination of samples from such sites in terrestrial laboratories; 5) deep access, probing through the ice shell into the ocean below, for in situ sampling and/or sample acquisition for return; 7) under-ice exploration of the top of the ocean; 8) submarine exploration of the open ocean including potential seafloor hydrothermal activity.

Figure 5 shows how all these steps cluster into three generations of exploration capability, of which Europa Clipper embodies the first.

Enceladus

Enceladus, a target of NASA's competitive New Frontiers program, is known already to be habitable (Figure 6). The multi-part definition used today to suggest that a world might be habitable are all met by Enceladus: global quantities of liquid water, salty, alkaline, long-lived; warm; with energy available from chemical gradients; organic chemistry (Figure 7). Cassini, an incredible discovery machine whose mission ends in Sep 2017 (Figure 8), made all these discoveries about the Enceladan ocean, most of them by first discovering that Enceladus is split at the south pole by four great fissures (Figure 9). They are much warmer than the surrounding icy terrain (Figure 10), a fantastic landscape of shapes made of ice and snow (Figure 11). Out of these fissures come about a hundred jets (Figure 12), water vapor geysers that loft solid grains out of the ocean. The largest grains are frozen ocean water droplets from the frothing liquid-vacuum interface in the conduits below. Lab tests of the droplet formation physics show that it concentrates organics. Many grains fall back to the surface to form the landscape. Others are

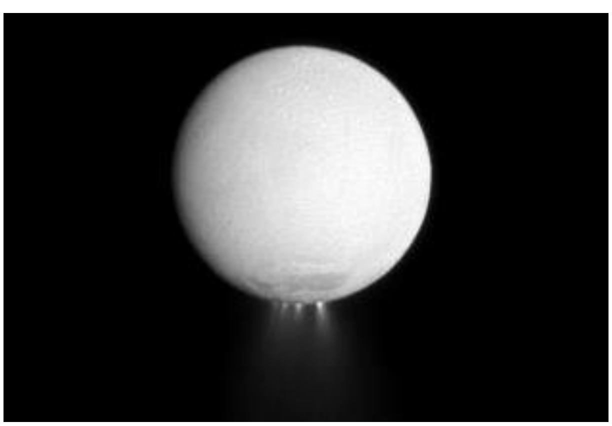

Figure 6. Enceladus spews material from its global ocean out into space where it has been sampled directly. Image: NASA/JPL/Space Science Institute.

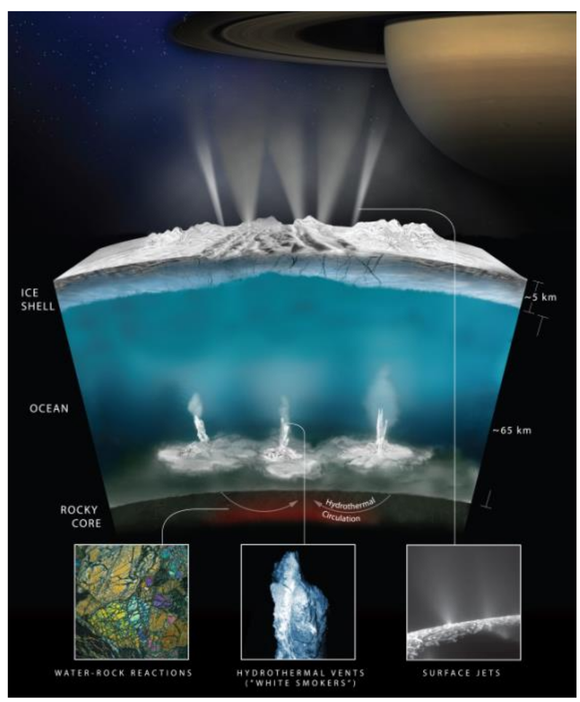

Figure 7. Enceladus is habitable by today's definition, given multiple lines of evidence measured by Cassini.

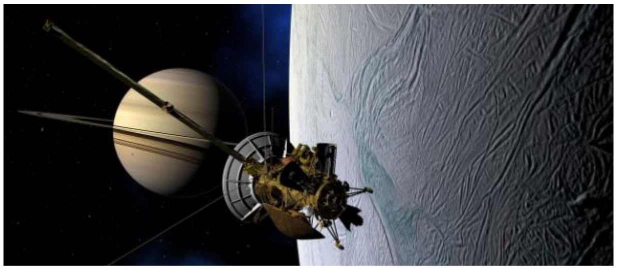

Figure 8. Cassini discovered that Enceladus is a habitable ocean world and demonstrated how to analyze its chemistry. Image: David Robinson / Bambam131.com.

Figure 9. Four tiger stripe fissures bracket the South Pole. *Image: NASA/JPL/Space Science Institute.*

carried by the vapor jets into a vast plume that extends far out into space, where they can be sampled directly with precise instruments. Cassini demonstrated how to do this.

Enceladus offers direct access to material known to originate in an environment known to be habitable. It is the easiest place to start directly searching for life elsewhere, via plume transects as flown by Cassini but with contemporary compositional analyzers. Subsequent steps could include: 1) soft capture of enough plume-grain material to apply developmental wet-chemistry

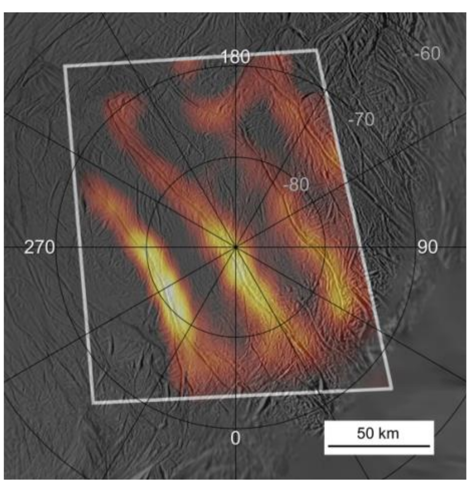

Figure 10. The fissures are much warmer than the surrounding terrain. [5]

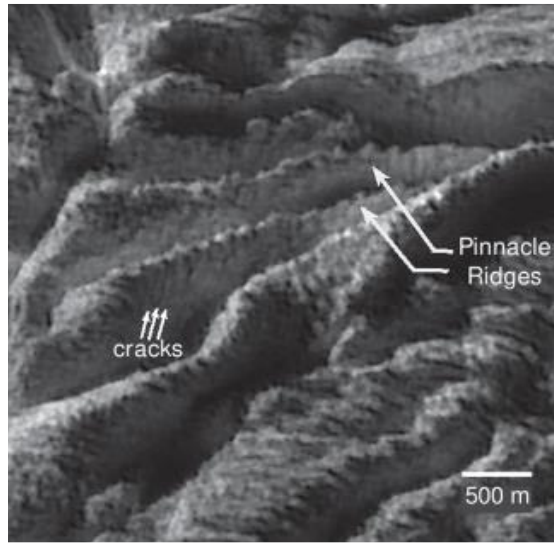

Figure 11. Highest resolution images of Enceladus show complex, jagged surface in vent region. [6]

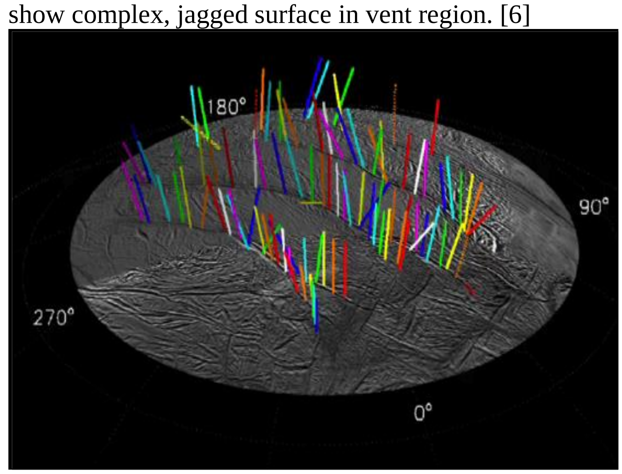

Figure 12. More than 100 jets form a broad plume extending hundreds of kilometers above the surface. [7]

and microscopy techniques; 2) collection, preservation, and return of samples for analysis in terrestrial laboratories; 3) accessing surface deposits adjacent to vents to collect large amounts of material for in situ analysis and/or sample return; 4) ‘downhole’ or drilled vent exploration to reach the liquid-vacuum interface; 5) under-ice exploration of the ocean ceiling; 6) submarine exploration of the open ocean, including the hydrothermal systems known to be there [4].

Figure 13 shows how these steps cluster into three generations of mission type. The first is under consideration by the New Frontiers program; a mission selected in 2019 could yield results as early as 2035. New instrument, payload, and sampling technologies could likely enable medium-scale missions including New Frontiers to perform the second group.

Titan

Titan, half-again larger than Earth’s Moon and another target of NASA’s competitive New Frontiers

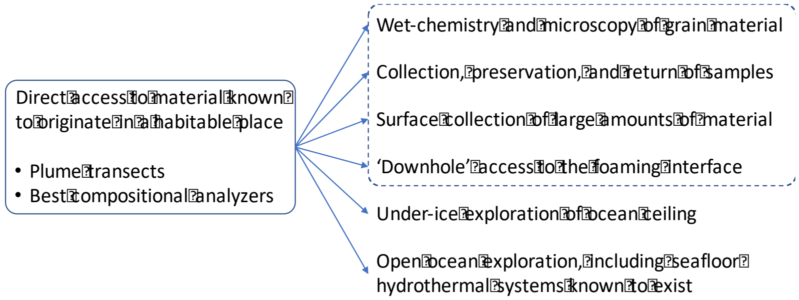

Figure 13. Enceladus program in three steps. So far, Enceladus meets known definitions of habitability, and presents ocean material directly into space, providing a way to detect biosignatures outright (solid box). Advanced capabilities would allow comprehensive study of the ocean's organic contents up to microbial scale (dashed box). Remaining goals would require development of several mission-class technologies.

program, is the solar system's 'organics factory' (Figure 14). It too has a deep global water ocean, whose composition is unknown but whose bottom may be in contact with silicate rock (Figure 15). The ice crust is thick, but atop and above it is a complex hydrocarbon world (Figure 16). The dense nitrogen atmosphere extends far into space. High up, sunlight just one percent as strong as at Earth is captured as chemical bonds by the formation of organic molecules, which grow into large haze particles that settle continuously onto the surface, forming the landscape along with the water ice, which is hard as rock at the temperature of liquid methane. Methane rain weathers this nitrogen-bearing organic sediment (Figure 17), pooling into large lakes and vast, deeply cold hydrocarbon seas (Figure 18). At suspected spots, cryovolcanic systems may warm the organic mix to liquid water temperatures for thousands of years; melt-pools from meteorites may last as long. At other suspected spots, subduction may be dosing organics directly into the interior water ocean.

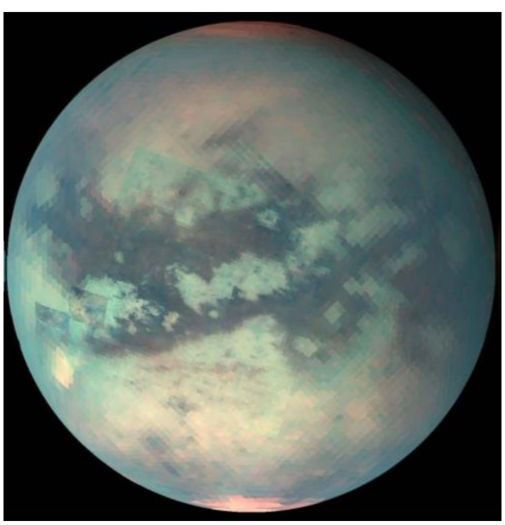

Figure 14. Titan is shrouded in a thick nitrogen atmosphere that supports complex photo-organosynthesis. The surface can be imaged through 'windows' in the infrared. *Image: NASA/JPL/Univ* AZ/Univ Idaho.

Titan is the best place to learn how different life might be from the life we know. Its size, geomorphology, methanologic cycle, weather, climate, organics lifecycle including capture of solar energy, and 'two worlds' configuration make it one of the most complex solar system bodies to explore. Many missions of multiple types would be required to understand Titan as a planetary system, let alone its potential habitability and its state of organic evolution toward biology.

Cassini performed 127 targeted flybys in 13 years, which made the fundamental discoveries that prioritize it as an astrobiology target. These flybys broadly constrained the interior structure, imaged swaths of the surface in radar and infrared, observed weather, obtained a few bathymetry profiles, sampled atmospheric organics up to 100 u and detected hints of much more. The Huygens probe obtained stunning descent images of complex drainage networks and shoreline systems, an atmospheric structure profile, and cm-scale images of the surface at its landing site.

What is needed next is comprehensive reconnaissance of this extremely complex world: a detailed inventory of the stratospheric organics factory

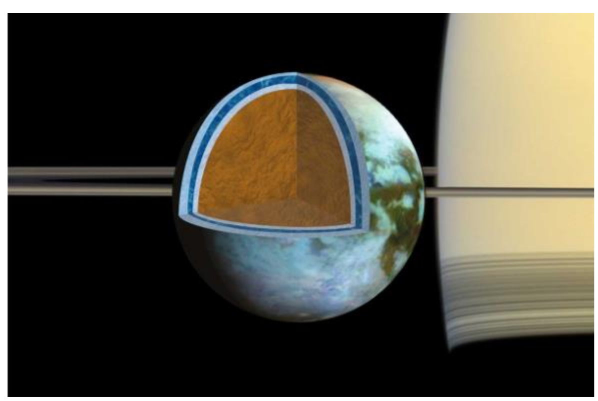

Figure 15. Titan also has a global water ocean enclosed within a thick ice shell; the seafloor may be in contact with silicate rock. (Proportions correct for hydrated silicate core.)

to reveal what is being made and how; global mapping

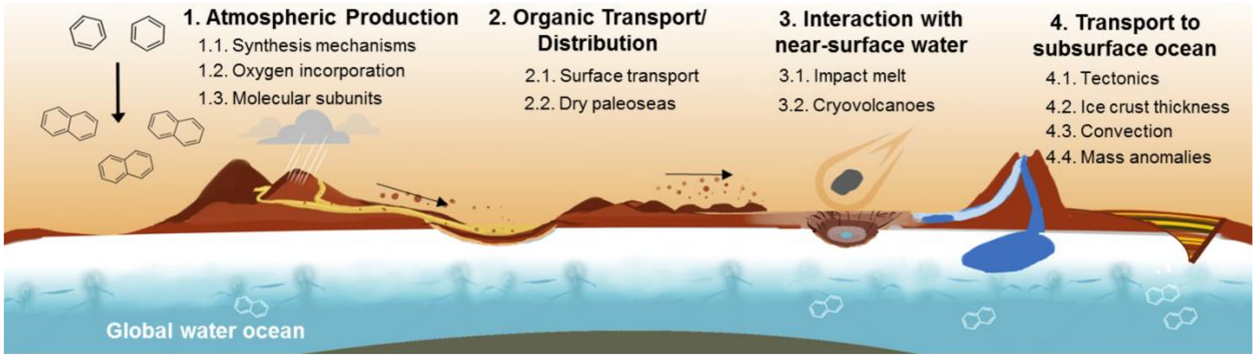

Figure 16. Titan surface is a complex hydrocarbon world, where the landforms are made of organic sediment from the sky, deposited over an ice crust and eroded by methane rain and rivers.

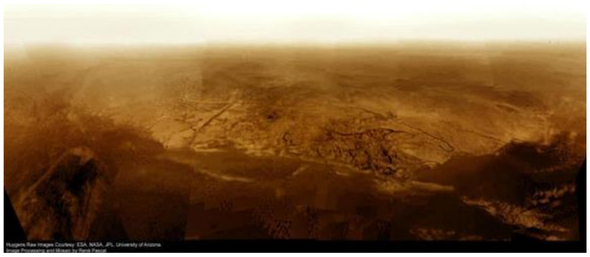

Figure 17. Global-scale methanological cycle erodes Titan's organic sediments, draining the weathered by-products into lakes and seas. Image: René Pascal/ ESA/NASA/JPL/University of Arizona.

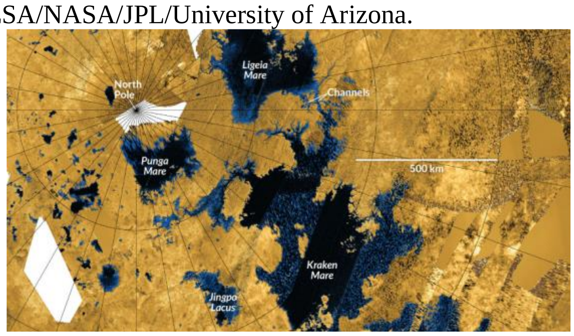

Figure 18. Surface methane-ethane seas collect the weathered organics. Image: NASA/JPL-Caltech/ASI/USGS.

surface weathers and where organics might come into contact with liquid water, and global tidal signature, gravity, and topography to determine how thick the ice shell is and whether the ocean is in contact with silicate rock at its base. Subsequent steps could include: 1) aerial exploration beneath the thick haze; 2) buoyant exploration of hydrocarbon seas; 3) mobile surface exploration of geologically and astrobiologically promising locales; 4) in situ analysis of the chemical fate and astrobiological state of weathered organics on the surface, particularly in areas where contact between organics and water can be inferred; 5) eventually, return of samples for analysis in terrestrial laboratories and 6) access into the interior ocean through the cryogenic ice crust.

Figure 19 shows how these steps cluster into three generations of mission capability, each encompassing several missions' worth of exploration projects. New Frontiers could take the first step and much of the second; a mission selected in 2019 could by 2035 start opening this cold, alien world to informed exploration by subsequent in situ investigations.

Taken together, the three primary ocean worlds give us places to search for extant life, complex life, pre-life, and even weird life. Thus they constitute the core

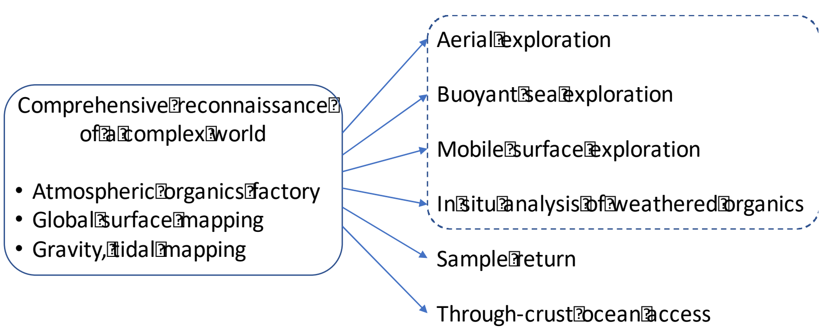

Figure 19. Titan program in three steps. Next to Earth, Titan is one of the most complex worlds in the solar system, easily proffering a century's worth of exploration missions. Next we could map its globe inside and out, and directly sample its organics factory (solid box). This would reveal where best to invest in situ missions, using technologies that could be flown today (dashed box). Several major mission-class technologies would be required for the remaining goals.

at high enough resolution to determine how the dynamic mission agenda for empirical astrobiology. While pursuing all three stages of exploration at each world would take more than a century's worth of advanced technology development and deep-space missions, the first 'next steps' could be accomplished at all of them by the mid-2030s, in a carefully structured program.

Ocean World Exploration Program Today

The US Congress directs NASA to implement a "virtual" Ocean Worlds Exploration Program using a mix of mission classes and types [8]. The next strategic milestone will be the emergence in 2022 of US planetary Decadal Survey priorities that succeed the current Decadal Survey Vision & Voyages [3].

In the meantime, several actions to meet the ocean world exploration challenge are in motion. NASA is developing the Europa Clipper multi-flyby habitability reconnaissance mission in time for a 2022 launch of the SLS [9]. NASA is also studying concepts for a second large mission, a Europa Lander that would search for biosignatures within the first 10 cm at a site (identified by Europa Clipper) as early as 2028 [10]. To meet that date, a second SLS launch would be needed, and instrument selection would need to occur in 2018; a Community Announcement for potential instrument selection preserves this option [11].

ESA is developing the large-class mission JUICE (Jupiter Icy Moons Explorer) that will enter orbit around Ganymede by 2032 [12]. High-capability instruments applicable to ocean worlds are developed across Europe, funded by the respective national agencies.

NASA also continues soliciting PI-led mission proposals. The agency added an Ocean Worlds Theme to the Decadal-specified list of five candidate science objectives for the New Frontiers program, NASA's billion-dollar class competitive mission opportunity, in early 2017. The NF-4 competition is currently evaluating two Enceladus concepts (ELF and ELSAH) and two Titan concepts (Oceanus and Dragonfly) that pursue various nodes on the roadmaps outlined above. Concept downselect is scheduled for Nov 2017, and mission selection for mid-2019, for a launch by 2025 [13]. Science return would commence in about 2035. Ocean-worlds concepts have not been tested in the New Frontiers selection environment until now, and selection of the subsequent NF-5 mission may or may not occur before the next Decadal Survey sets priorities.

The smaller (roughly half-billion-dollar), more frequent Discovery mission opportunity is challenged to offer enough science to merit the likely cost to NASA: since Discovery 2010, three ocean-worlds mission concepts have washed out: first JET, then TiME at the end of a Phase A study, then the original ELF in 2015. Advancements since those nonselections, funded both internally by proposing institutions and by NASA MatISSE and COLDTech [14] instrument and technology awards, are feeding into the current New Frontiers competition and will inform any mission concept studies commissioned by NASA in 2019 to support the community Decadal Survey process.

Ocean World Program Stakeholders

The many activities underway – from technology to instruments to formally proposed concepts to missions – comprise a 'virtual' ocean worlds exploration program: a virtual OWEP. The advantages of this as a strategy include the continuous, status quo exercising of competitively sharpened science and formulation communities. However, this community now has the potential and capacity to prosecute a richer, more organized and orderly program, given that an implementing community is building around the Europa Clipper project and Europa Lander Pre-Project. If an important strategy criterion were 'the fastest way to learn the limits of life,' then faster options are available; however these would likely require the virtual program to be a real one. Line-item programs gain a coherence in the minds of sponsors and participants alike that could focus and unleash the talent already built up. Doing this could vault exploration of the ocean worlds forward.

In only 15 years, the Mars Exploration Program (MEP) has made diverse, deep scientific and exploratory contributions to collective knowledge of the cosmos. This remarkable record demonstrates how sustained investment within a structured program can assure both a healthy science and technical workforce and stepwise progress on a strategic roadmap. The MEP has extended human experience and knowledge outward to a frontier keenly relevant to how we see ourselves in the cosmos. Such places loom large in our vision of humanity's future. The quest to know if life ever existed on Mars is now convolved with anticipating a human future there, a milestone that could happen in this century.

But the scientific context has changed: now we know that Mars is our solar system's large exemplar of a paleo-relict ocean world. Ceres turns out to be Mars' smaller counterpart: Dawn revealed the inner solar system's only dwarf planet also to be a relict ocean world [15]. How long do ocean worlds last? How far do they progress toward life? Perhaps even, how does life adapt as habitable conditions fade?

In the case of our solar system's more distant ocean worlds, with large extant oceans, humankind's only access deep into the future will be by avatar: Jupiter imposes a hazardous radiation environment on the Europan surface; Saturn is a half-decade away even with an SLS-class launch. Through sophisticated telepresence and advanced in situ instruments, we can learn how habitable conditions may arise, vary, and be sustained, and how far they may have progressed toward complexity and life. 'Following the water into the ocean worlds' is the best way to learn whether another part of our universe is also alive.

Strategy Analysis: Lessons from the Mars Exploration Program for Ocean Worlds Exploration

The following strategy analysis demonstrates that a virtual OWEP cannot match the focus, velocity, or persistence of a program as successful as the MEP has been. A 'middle class' of directed-purpose missions would be needed to guarantee a steady, orderly cadence of key results, which in turn would be the foundation upon which strategically defined flagship explorers can build complex, empirical planetary science.

Measured by its pace and depth of hypothesis-driven scientific discovery, its sustained progress and longevity, its support by and of large science and engineering communities, and the reach of public interest about it – all of which have boosted the NASA brand – the MEP has achieved a kind of success that must be understood for applicability as a template for other planetary exploration programs including a potential OWEP.

Yet six MEP-enabling conditions either do not or cannot be ported directly over to an OWEP that starts with the three highest-priority ocean worlds; the lessons must be adapted to be constructive.

1. Mars-class missions are technically moderate in many dimensions. After the Moon and Venus, Mars is our nearest planetary neighbor, at the outer edge of our sun's 'habitable zone'. One-way light-time from Earth ranges from 3–20 minutes; sunlight is 44% as strong as at Earth, and the Mars night lasts nearly the same as Earth's (the diurnal period is 24h 39m 35s). Surface temperatures range between 130–308 K. The CO2 atmosphere is dense enough to use for entry and descent deceleration, albeit thin enough that a succession of technologies is needed to reach the surface safely; landing at the higher altitudes in the southern highlands has not been demonstrated. Critically though, the surface can be explored from orbit in all wavelengths.

Sunlight at Jupiter and Saturn is, respectively, only 3.7% and 1% as strong as at Earth. Solar power is feasible for orbiters, as demonstrated by Juno. Landers for the icy moons are harder to design for solar power: the Europa and Enceladus days last 3.55 and 1.37 Earth-days, respectively. Titan's day lasts almost 16 Earth-days, and its hazy atmosphere absorbs 90% of the feeble sunlight anyway, so extended surface operations would require radioisotope power.

Titan has the most benign atmosphere in the solar system for entry, descent, and atmospheric flight: dense, extended, nitrogen. However, Europa and Enceladus are airless, so propulsive descent is required; Enceladus is quite small, with only about 1/87 g at the surface. By contrast, landing on Europa is like landing on Earth's Moon; it has about 1/7 g; in addition, it orbits inside Jupiter's strong radiation belt, receiving about 540 rem/day.

Key Mars science, albeit not extant life, is accessible on the surface. The consensus is strong that any prospect of finding life on Mars requires getting deep: into aquifers, paleo-volcanic areas including lava tubes, and down boreholes. However, being a desiccated ocean world, Mars has given humanity a tremendous adventure already, through rocks exposed in key places on the surface which may contain paleoindicators of life.

The Jovian and Saturnian ocean worlds are icy moons: their liquid oceans lie beneath kilometers-thick ice crusts. Our natural exploratory urge is to gain access to the potential habitats inside, as rapidly as possible. Yet it is also likely that these moons' surfaces can give us rich exploration adventures along that quest, akin to those at Mars, and far earlier than ocean-access missions themselves can become practical. Operations complications hidden by thick ice are not the only challenge: on their surfaces, the Jovian and Saturnian icy moons are not just icy, they are cryogenic. Europa ranges from 50–110 K; Enceladus ranges from 33–145 K; Titan is about 94 K. Such cold conditions, in which ice is as hard as concrete, challenge the performance of many aerospace materials, mechanisms, and components.

The atmosphere and surface of Titan are nonetheless accessible within New Frontiers resources; global reconnaissance may reveal surface sites where complex organics interact with the interior ocean on astrobiologically relevant timescales. Should the surface of Enceladus become a high priority, it is probably also accessible within New Frontiers resources: it lacks the deceleration benefit of an atmosphere like Titan, but it has low gravity. The largest uncertainty is surface mechanics in the dramatic landscape around the fissures, near the vent openings; without in situ data we cannot know whether the plume deposits are hard ice or 'cotton candy'. We also lack imagery at the scale of a lander, so requirements cannot be developed for obstacle avoidance. Europa is hardest of all: its large gravity and Jovian radiation environment make landing and functioning on the surface quite challenging. But here we expect a tectonically controlled landscape, with large quantities of ocean material embedded in the cryogenic ice.

Not one of the three primary ocean worlds can be reached or explored as easily as Mars. This gap poses a significant, inescapable contrast between the MEP and any OWEP. Some of the challenges, like cryogenic operations and modular radioisotope power systems, are common to all icy moons, and are therefore amenable to cross-program technology investment.

2. 26-month synodic period, and half-year transfers, allow Mars exploration to respond to new knowledge with new missions on a half-decade cadence. Ballistic transfer takes less than once around the sun, and a Hohmann transfer window occurs at just over two-year intervals. This good fortune of celestial mechanics allows us to develop missions with a half-decadal periodicity, each based directly on fresh technical experience and the emergent science obtained.

Reaching the Jovian and Saturnian ocean worlds is harder and takes longer, and therefore costs more on average. Standard expendable launch vehicles impose half-decade (to Jupiter) or decade-long (to Saturn) transfers due to multiple gravity assists in the inner solar system, which in turn drive the hot limit for flight components. (The fastest chemical propulsion trajectories to Saturn take seven years, given a Jupiter gravity-assist that is available only every 18 years.) Very large rockets in development today, like the SLS and Falcon Heavy, offer NASA the potential to halve these trip times and avoid the complications of an icy-moon spacecraft flying inward around the sun first; so too would a high-power SEP (solar electric propulsion) boost tug such as those available from today's private sector.

Beyond these two sets of constraints that nature provides, choices are up to us.

3. Extra-project investments can assure development of enabling technologies that are key to strategic progress. A mission series allows systematic investments in key technologies to be amortized over several missions, without fully burdening any one of them or subsuming the technologies' requirements to singular mission goals. The MEP tried to maintain such an MTP (Mars Technology Program) as a dedicated program budget line item, but strategic technology budgets and early infusion are notoriously difficult to protect and assure, given the focus on tactical requirements that all projects develop. Nonetheless, key capabilities developed by MTP and its successor investments, such as software-defined radios, EDL (entry, descent, and landing), rocker-bogey roving, and radioisotope power generators that operate in the Mars atmosphere, have been essential to MEP success.

Without a sustained technology program, a virtual OWEP would lack the ability to make multi-year investments in strategic core technologies. COLDTech is the virtual OWEP's seed of an OWTP (ocean worlds technology program); in its first year COLDTech distributed about $25M across 16 competitively awarded, two-year maturation initiatives [14]. This first round of small investments is quite diverse: some could be used at multiple worlds, some are singular, and some are aimed at missions far down any reasonable roadmap. Achieving project-ready TRL would require increased per-technology funding, growing COLDTech significantly or down-focusing its portfolio, or both. Either would require a top-down strategy difficult to reconcile with today's competitive selection process.

A strategic OWTP could establish metrics-driven capability advancements applicable to, and needed at, all three primary ocean worlds: 1) autonomous exploration that can conduct branched, open-ended science investigations despite hours-long communication delay with Earth, or in conditions where Earth control is not possible at all; 2) planetary protection of potential habitats, and of Earth from returned ocean-world material; 3) integrated protocols for making life-detection retrievals, including measurement techniques not yet flown in deep space; 4) acquisition, handling, and preservation of astrobiological samples in forms ranging from rock-hard ice to clathrates to liquid water to volatiles; 5) mechanisms and electronics that function reliably in cryogenic conditions, including without altering the state of the native material; 6) modular radioisotope power systems for landed and submerged mission operations in potential habitats.

Such strategic investments could systematically mitigate the intrinsic challenges of exploring ocean worlds described in the previous section; without them, each first mission would have to absorb – and be prey to the parallel schedules and cost uncertainties of – maturation of all its enabling capabilities.

4. Ongoing operational infrastructure 'lowers the bar' for individual missions. A top program priority throughout the history of the MEP has been sustaining telecom relay assets in orbit. Today, multiple spacecraft provide redundant relay links in the Mars network, which keeps the surface of Mars in touch with the DSN (Deep Space Network) on Earth. Mission-critical events like MOI (orbit capture) and EDL are also covered fully, providing technical confidence for each next mission in the program. The assets' orbits are routinely shifted to assure best coverage, making this a flexible, albeit aging, infrastructure.

The Mars assets do more than link data nodes. Each also carries a sophisticated science instrument payload. The whole surface has been mapped in infrared (mineralogy), thermal emission (dust, soil, rock, ice), and visible (morphology) wavelengths. About 1/40 of the surface is imaged at half-meter pixel scale. Altogether, and deeply enhanced by human-scale scenes provided by roving surface avatars, this comprehensive exploration of the planet has made it feel already like another home for humanity.

Confronted by a multi-world exploration imperative, an OWEP would require a different type of infrastructure than the MEP. Single-world infrastructure might be justified in the future by discoveries not yet made, but the highest scientific priority now is to take the next steps at each of the three primary ocean worlds. Trip time tops the list for all three, based on celestial mechanics. Thus the first most-enabling common infrastructure for an OWEP would be high-capacity propulsion into the giant-planet systems.

For example, with a solid-rocket-motor kick stage, SLS performance could deliver roughly 2000 kg into Saturn orbit only four years after launch. A conventional launch vehicle (e.g., Atlas V), with a 25-kW SEP in-space stage, could deliver the same mass with a 5–6 year flight time. Such SLS direct-launch and SEP-enabled opportunities occur every year. The SLS and Falcon Heavy super-heavy launch vehicles are expected to begin flying within a few years; 20–25 kW commercial SEP spacecraft are already flying.

5. The agency determines MEP project new-starts within a single program budget line. This administrative structure simplifies the agency's duty to manage its portfolio of projects, to get the most value from public money while making strategic progress. Although Congress can provide direction about individual projects, dividing the budget at the level of programs leaves the agency maneuvering room within which to manage, during the course of each fiscal year.

For planetary science, NASA has multiple programs: MEP, ExoPlanet Exploration (in the Astrophysics Division), Cassini (a single-mission program), the competed opportunities New Frontiers, Discovery, and SALMON, and a large portfolio of research, instrument maturation, and technology development programs. The Cassini Program was responsible for uncovering the potential of two of the three primary ocean worlds, but the mission itself ends in Sep 2017, and the program along with it. A line-item OWEP, supported by an OWTP, would allow the agency to guarantee a predictable rate of progress on the quest to understand life.

Creation of a line-item program requires significant stakeholder commitment, because the degrees of freedom obtained for the target program may be purchased at the expense of degrees of freedom for other types of planetary science. The current Decadal Survey rated the return of scientifically selected rock-core samples from the relict ocean world Mars at the top (for implementation by the MEP); followed by reconnaissance of the primary ocean world Europa to determine its habitability (planned for implementation by the Europa Clipper); and then comprehensive orbital study of either ice giant (which would also allow reconnaissance of apparent ocean worlds among the icy satellites of both Uranus and Neptune).

Today, missions to conduct the balance of planetary science are selected within the competitive programs. The forum that sets the balance among priorities across all of planetary science is the Decadal Survey, a complex community engagement process. The next survey is scheduled to culminate in 2022. It will consider discoveries at Enceladus and Titan not yet made when the current survey was cast; the evolving state of knowledge from hundreds of related research papers published in the meantime; capability-driven mission concepts developed by NASA from FY16 through FY20; and published planning analyses. It will issue a detailed, vetted assessment of the state of knowledge of planetary science as of 2022, an assessment of what might be feasible within the 2023–2032 decade, and guidance for how NASA should allocate science objectives for that decade among flagship, medium-scale competed (New Frontiers) and small-scale competed (Discovery) program opportunities. A prioritized list of directed missions and a non-prioritized list of New Frontiers objectives would be expected.

The development of integrative strategic themes, a major decision in decadal surveys, may help pave the way for a line-item OWEP, or not. The current survey predates today's better-informed, broader conception of ocean worlds. Thus a critical milestone for any OWEP will be the 2022 release of the next survey.

6. Directed missions in the \$0.5–1B class constitute the MEP's essential connective tissue between flagship missions. Table 1 lists the US Mars mission projects since Viking*. Punctuated by flagship peaks, a cadence of small-to-large missions has propelled the rapid scientific exploration of Mars. Most of these were directed missions: NASA chose sets of functions and measurements that would steadily advance the front of scientific knowledge, while laying in infrastructure, demonstrating capability, and learning facts that helped the flagship missions be designed and succeed. The rate of startling discoveries at Mars is proof that such a strategically defined program can be a potent tool for progress.

The MEP foundational missions cost \$0.25–1.1B in today's money and were developed to exploit launch opportunities occurring every 26 months. Eight of them were launched in the 13 years from 1992 to 2005. They supported the MER twin rovers, the flagship Curiosity geochemistry rover, and are expected to support the flagship Mars 2020 rover. They enabled the competitively selected science missions Phoenix,

* All costs discussed in this paper are in \$FY17, except as noted.

Table 1. Mars Exploration Program in four parts. Scientific infrastructure missions (gray) and lessons from early failures (orange) built the foundation both for challenging in situ exploration (green) and for cost-constrained, competitively selected missions (tan).

Project	Launch	Rough Cost* ($B FY17)
Mars Observer	1992	1.14 [16]
Mars Pathfinder (Discovery)	1996	0.48 [17]
Mars Global Surveyor	1996	0.34 [18]
Mars Climate Orbiter Mars Polar Lander	1998 1999	0.35 [19]
Mars Odyssey	2001	0.59 [20]
Spirit and Opportunity, MER Rovers A & B	2003	1.13 [21]
Mars Reconnaissance Orbiter	2005	0.77 [22]
Phoenix (Scout)	2007	0.56 [23]
Curiosity (MSL)	2011	2.43 [24]
MAVEN (Scout)	2013	0.56 [25]
InSight	future	0.63 [26]
Subtotal as of 2017		**~ $9B FY17**
Mars 2020 rover	future	future
“NeMO” Next Mars Orbiter	potential	future

* Variable assumptions (see references); use only for rough sense of scale.

MAVEN, and InSight to enrich and exceed the ‘Follow the Water’ strategic thrust at the core of the MEP.

Equivalent strategic authority to direct a line-item OWEP would likely require creating a class of directed-purpose missions whose resources (~$1B) could be commensurate with ocean-world challenges. Continuous development of a series of these missions, and the spacecraft and payloads for them, would sustain engagement, motivation, and focus by a healthy community of American ocean-world science and engineering talent (directly related to the American oceanographic, aerospace, and robotics talent pools). The next key exploration objectives for each of the primary ocean worlds would be assured, lending stability and velocity to the program and thence to the cadence of science revelations among the ocean worlds.

Planning Options for an OWEP

Options can be considered that span incrementally from today’s virtual OWEP to a formally planned OWEP that adopts lessons from the successful MEP.

Case 0 is the default state: Europa Clipper flagship mission, with results starting in the mid-2020s; potential Europa Lander flagship mission, with results as early as ~2030; potential ocean-world-themed New Frontiers mission selected in 2019 (at 4/12 odds in Step 1) from the four currently under evaluation by NASA (two for Enceladus, two for Titan), and with results starting in 2035–36. Note that the term of the next Decadal Survey ends in 2032.

Specific technologies and measurement techniques proposed individually by members of the community might be developed, demonstrated, and matured by periodic COLDTech, PICASSO, and MatISSE investments.

This case recognizes that flagship priorities are set by Decadal Surveys. Today’s three-mission concept for implementing the current survey’s top planetary flagship priority (progress toward Mars Sample Return) sets a precedent for a two-mission plan to implement its second priority (Europa habitability), which would be reflected by the combination of the planned Europa Clipper and the Europa Lander concept under study. However, this multi-mission plan was not discussed by the current survey, so it is unknown whether the mid-Decadal review underway now, or the next Decadal Survey, will rank a Europa Lander mission as more or less important than the 3rd, 4th, and 5th flagship priorities written into the current survey. A virtual OWEP must accommodate the explicit Decadal Survey guidance to maintain a balanced program.

Other, hypothetical cases systematically increment resource commitments beyond the default case. Figure 20 shows that these options are not strictly additive; rather they provide multiple entry points.

Case 1 would select two New Frontiers missions in 2019 from the 12 in the current competition. This option could avoid disrupting the continuity of other planetary science, while also achieving a fast start on a multi-world OWEP. Since the two primary ocean worlds at Saturn are in the current competition, a selection in 2019 would take a next step at one of them, according to the priorities proposed by its PI and science team. With this option, NASA could make simultaneous progress at multiple ocean worlds (Europa and one of the others) without perturbing the existing

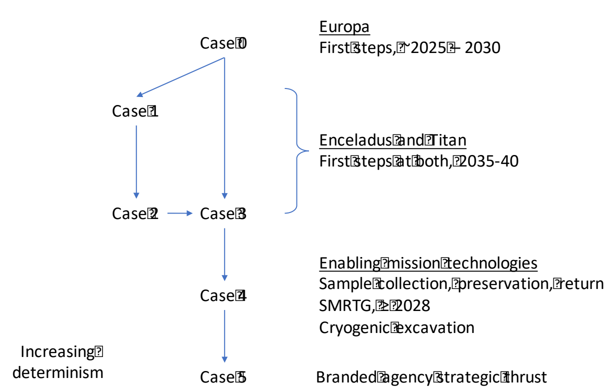

Figure 20. Hypothetical program scenarios are logically linked, yielding stepwise gains of strategic control over the content and pace of an OWEP.

program frame-work, for the additional cost of a single mission (about $200M per year for half a decade; see Case 2 below).

Not being selected would shift attention to the subsequent NF-5 competition about a half decade later, and put the competition for an ocean-worlds mission on the cusp of the next Decadal Survey. If begun before the survey's release, the NF-5 competition might reduce equal-priority science themes from six to five depending on what is selected in 2019, but would add two more already specified by the current Decadal Survey, thus diminishing the intrinsic odds of an ocean-world selection. The next Decadal Survey will likely renovate the list of NF science priorities in 2022.

Case 2 would establish a pattern of selecting two New Frontiers missions in every opportunity. This option would effectively double the capacity of the New Frontiers program, committing the expanded half to ocean-worlds exploration. Doing so would enable NASA to answer the most critical, urgent questions about all three primary ocean worlds by roughly 2040. Is Europa habitable? Where and how can we gain access to ocean material? Does the Enceladan plume contain biosignatures? Is surface access around the Enceladus vent openings trafficable? Does the organic photosynthesis in Titan's upper atmosphere incorporate oxygen? Are there places on Titan's surface where processed organics may interact with liquid water?

This option could launch two ocean worlds missions roughly every decade. Since 2003, the New Frontiers program has cost $204M per year [27]. Roughly $200M per year more than today's program would assure the technical community an essentially continuous development of ocean-world missions, and assure the science community steady progress in the exploration of ocean worlds. In a competitively awarded program mode, this development work might move around the country from build to build, a challenge for continuity of expert workforce, and the specific science achievements would depend on the priorities proposed by PIs and their teams rather than by the community through an integrated, accepted roadmap.

Supporting either Case 1 or Case 2, proposing mission teams would continue developing competitive capabilities by petitioning programs like COLDTech, PICASSO, and MatISSE to mature instrumentation and technology for their own mission concepts.

Case 3 would create a $1B directed-purpose mission class. Experience with the Discovery competitions in 2010 and 2014 indicates that ocean-worlds missions are generally impractical at the half-billion-dollar level, but should be able to deliver significant investigations at approximately the billion-dollar level. If able to direct a strategic sequence of pivotal combinations of science objectives and technology demonstrations at this scale, NASA could propel the quest for life in the ocean worlds with velocity and effectiveness comparable to the MEP's exploration of Mars.

There are two ways to do this. First, the objectives and demonstrations could be written into the requirements of successive New Frontiers opportunities (a la Case 2), for competitively awarded implementation. That is, 'directed purpose' need not equate to directed missions. Or, NASA could simply assign a series of directed-purpose NF-class missions, as it has all the MEP foundational missions. In either scenario, scientific and technological progress would be assured of following the priorities of a strategic roadmap.

In the directed-mission scenario, as with the MEP, separately proposed, competitively awarded, PI-led New Frontiers missions could augment the strategic thrust to 'follow the water into the ocean worlds'. The Phoenix, MAVEN, and InSight missions augment the MEP with investigations of surface geochemistry, confirmation of near-surface ice, aeronomy of the upper atmosphere, and interior geophysics. However, if the Planetary Science Division budget were realigned to support a series of directed-purpose, NF-class missions to implement a line-item OWEP, it is reasonable to expect that the broader planetary-science community would advise NASA to exclude additional ocean-world proposals from the competitive New Frontiers program in the interest of assuring scientific balance across solar system themes.

This option might cost as little as Case 2 (i.e., ~$200M/yr) for two NF-class missions per decade. But it could cost up to roughly twice as much if multiple directed-purpose objectives get aggregated into small-flagship-class missions.

Even so, the actual rate of progress would be technology-limited rather than mission-limited, as advanced capabilities for operations, sample access and handling, and instrumentation would still be subject to the uncertainties of competitive programs like COLDTech, PICASSO, and MatISSE in which selections are a function of the objectives proposers submit, the quality of their proposals, and the assumptions made by evaluators, not just a Planetary Science Division strategic plan.

Case 4 would add a strategically managed ocean worlds technology program. This penultimate step would give NASA budgetary and planning flexibility to invest in capabilities that are useful for multiple missions, maturing them at the right time to 'turn on' key nodes of an integrated roadmap. Sequestering the resources would be tough to sustain; technology program resources historically end up folded into project control, which reduces the infusion rate of advanced capabilities. Despite the daunting inherent physical differences between exploring Mars and exploring the primary ocean worlds, this option would equip NASA to pursue the most promising leads, in the most appropriate sequence, using the most applicable technologies, on its quest to find and understand life in the universe.

The well-honed evaluation mechanism for competitively awarded missions actively eschews new technologies, so without a programmed technology campaign, and perhaps also directed infusion of the results, overall progress would rapidly become technology-limited – the opposite of what an OWEP calls for. The formidable technical challenges involved in progressive exploration of interior oceans beneath kilometers of cryogenic ice on multiple worlds distributed across the outer solar system cannot realistically be met without a dedicated technology development program.

Some are unique to specific worlds (e.g., vent access at Enceladus, or radiation survivability at Europa). Some are cross-cutting for an entire OWEP (e.g., modular radioisotope power systems, or cryogenic components and mechanisms). And some are almost guaranteed to yield spinoffs outside of NASA (e.g., the capability for artificial intelligence to conduct a sophisticated science investigation out of contact with human controllers, based on what it learns from executing its own exploration decisions).

Using the MEP history as a guide, a robust OWTP program might be protected at about 10% over the OWEP budget, thus perhaps ~$50M per year in addition to the mission commitment of Case 3.

Case 5 would establish a formal OWEP. This final, structural administrative step would put the reins of the foundational missions and technology campaign, the championship of and responsibility for ocean-worlds flagship missions, the sponsorship of any competitive ocean-worlds missions and projects, and NASA OWEP brand management in the hands of an agency program executive.

In this option, designating a program office and assigning operations staff would maximize the efficiency of administering all the elements built up, as well as interface coordination with other organizations inside and outside the agency. It would advocate for the ocean-worlds scientific frameworks and priorities as they evolve, in agency planning and other decisions (e.g., definition of ROSES research categories) that directly affect many stakeholder communities – including geophysics, geochemistry, astrobiology, oceanography, organic chemistry, and other domains of space and life science; and mission, system, spacecraft, instrument, and laboratory providers. It would draft, coordinate, and maintain planning roadmaps for missions and technologies, to keep stakeholder alignment visible. It would synthesize emergent results with plans and decision points, and coordinate outreach messaging, representing to the wider world NASA's quest to find and study life in the universe.

Timeline Analysis

An OWEP would be scientifically complex and technically challenging. Especially given the cost in money and time to explore worlds in the outer solar system, it would also take a long time. Figure 21 shows a half-century snapshot of 'things that were, things that are, and some things that have not yet come to pass' in the exploration of the primary ocean worlds. Immediately apparent is that the Decadal timescale is inadequate for encompassing iterative mission cycles at any of these worlds.

A multi-generational program is needed to mount missions, reach the targets, conduct and assimilate the scientific investigations, decide how to respond, and plan follow-on missions. In advance of taking the most important next steps at each of the three primary worlds, we cannot know how discoveries may steer our objectives, perhaps focusing them on one of the worlds, or spreading them across two or all three.

Two tautological findings are clear from the figure: 1) Shortening trip time would increase the rate of informed progress; the early keys are super-heavy lift launch and high-efficiency in-space propulsion. 2) Results 'iron bar' to the right with flight-project start decisions; under the status quo, New Frontiers selections are the only degree of freedom, and they occur only every five years.

Conclusions

Many NASA missions – several by the MEP, plus

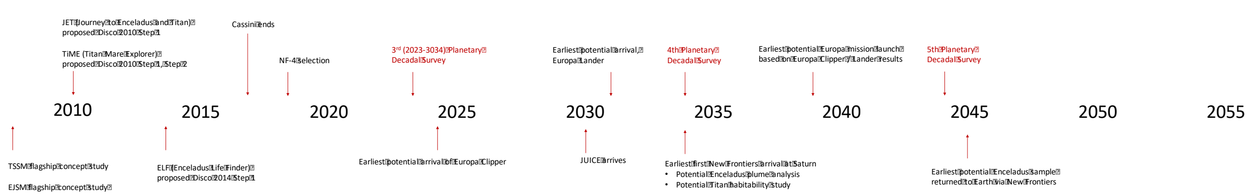

Figure 21. Half-century timeline of past, contemporary, and potential future OWEP milestones is constrained by status quo programmatics. Only a formal, stable, strategic program sustained over decades could deliver accelerated results in the quest to understand the potential for life in the ocean worlds.

Voyager, Galileo, Cassini, Dawn, and New Horizons – have identified a class of worlds not envisioned just a few decades ago: worlds with both extant and extinct liquid water oceans.

Of these, Europa, Enceladus, and Titan have the best combination of attributes, including documented oceans, surface liquids, and organic chemistry, sufficient to motivate a significant and achievable human quest: to tangibly learn the limits of life in places we can reach with 21st-century technology.

In this our century, we can know whether we are alone in the cosmos. Next steps toward exploring the primary ocean worlds are either already underway (e.g., Europa Clipper), under study (i.e., Europa Lander), or awaiting potential selection in the competitive New Frontiers mission program.

Beyond these steps, the strategy challenge for an OWEP is how to implement the most ambitious steps within plausible budgetary scenarios that preserve a balanced solar system exploration program, on timescales meaningful to practicing scientists, lawmakers, and the public. Significant progress could be guaranteed for about $0.5B per year, about 1/40th of the NASA budget. The payoff would be a fundamental step forward in understanding our cosmic uniqueness, an unprecedented investment in science and the humanities.

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